ened the fatal result, inevitable, perhaps, without it. The state which seems to have been most dreaded, and against which this most formidable remedy was employed, was congestion—the accumulation of dark blood in some part of the body, probably in the large veins of the thoracic and abdominal cavities, or in the vessels of the viscera themselves. How it was ascertained that the organs contained in these cavities were really in a congested state has not been mentioned; neither has the nature or the action of the primary cause effecting these accumulations or stagnations of the vital fluid been explained, nor as to how they gave rise to spasm, the flow of immense quantities of fluid from the intestinal canal, and, in the end, to death. The mere mechanical obstruction offered by these congestions did not interrupt respiration or the action of the heart; neither can we suppose they interfered with the secretion of bile or urine; and as none of the tissues of any of the organs, however much they may have been loaded with blood, were found after death to be disorganized, it is difficult to imagine why simple congestion should have been feared, and equally difficult to imagine how the abstraction of blood could be supposed to remove the primary cause effecting the obstruction of the circulation, or (unless by diminishing the quantity) relieve the overburdened organs of their surplus blood. The purpose for which blood was abstracted, appears to have been to excite reaction; but, if it really ever had this effect, it seems as if we might suppose that the increased force of the arterial current would likewise increase for a time the venous congestion, and thus the purposed remedy would add to the evil it was intended to remove."

The reliance to be placed in the observations made, and results exhibited in the report, may be estimated by the fact that the English squadron on the East India station, for at least ten years previous to 1840, seldom consisted of less than ten, or more than eighteen vessels, bearing a yearly average of from 1,500 to 2,000 men. During the war with China, the forces were increased to 7,000 and upwards. G. E.

ART. XVII.—*History of the Yellow Fever at New Orleans, La., in* 1853. By E. D. FENNER, M. D., one of the Physicians to the New Orleans Charity Hospital, &c. &c. New York: 1854. 8vo. 84 pp.

WHEN a disease breaks out in any locality, it would appear, at first sight, to be a very easy matter to settle the question as to its origin—whether from some cause existing in the locality itself, or a *materies morbi* introduced from without. And yet we find that, in reference to the occurrence of yellow fever in this, as well as in other countries, the question of its domestic origin or importation from abroad, is still a subject of much and often angry controversy. This, however, we apprehend, arises less from the intricacy and obscurity of the question itself, or from the want of sufficient data for its satisfactory solution, than from the one-sided view that has been taken of the circumstances connected with the several visitations of the disease. It is, unfortunately, too often the case that a few badly interpreted facts are seized upon, while others, of even greater significance, are either entirely overlooked, or, by a forced construction, are made to conform to the conclusions deduced from the incomplete or erroneous data that have been first adopted.

A vessel arrives at one of the wharves of a city from some foreign port—from one, perchance, within the yellow fever zone, and within a few days one or more of her crew are attacked with the disease, while shortly afterwards numerous cases occur in individuals residing in the immediate vicinity of the spot at which the vessel lies, and perhaps in others more or less remote. With many, such facts afford unquestionable evidence of the importation of yellow fever, which conclusion is almost invariably that adopted by the community at large, who never investigate closely or reason accurately, especially on questions relating to the etiology of disease, and whose mind it is always difficult to disabuse of any error it may imbibe.

The arrival of a vessel from a yellow fever locality, and the subsequent occurrence of that disease among her crew, and in those residing in the immediate vicinity of the wharf at which she lies, are not, of themselves, sufficient to prove the importation of the fever. Upon a closer investigation we shall, perhaps, find that the occurrence of disease on board the newly arrived vessel took place subsequent to her entrance into a locality where all the causes that are acknowledged to be capable of producing yellow fever already exist, and where, on other occasions, it has broken out without the presence of any vessel from either a healthy or unhealthy port, or the introduction from without of any source whatever of infection. It is, therefore, as reasonable to suppose that the crew of the vessel contracted the disease after her arrival, from the local causes there existing, as that by her means it was introduced amid the persons on shore simultaneously or subsequently attacked.

It will, upon further inquiry, be found, perhaps, that the greater number of the persons attacked had no communication, either direct or indirect, with the suspected vessel, and were out of reach of any poisonous emanations from her hold; while, at the same time, no fact connected with the occurrence of the disease countenances the supposition of its having been communicated to them by those primarily attacked.

Carrying the investigation still further, a most important circumstance may, peradventure, appear, namely, that new cases of the fever continue to occur in the same locality many weeks, if not months, after the removal or departure of the vessel to which its origin was attributed; or, perhaps, the still more startling fact will be discovered of the occurrence of sporadic cases previously to the arrival of the suspected ship.

That in all those portions of the United States where the yellow fever has occurred—and the remark may be extended to other countries—there exist local causes, sufficient, under particular circumstances, not yet well understood, to produce the disease independently of any foreign sources of infection, is a fact very generally admitted. In too many instances, the possibility of its domestic origin has been too clearly demonstrated to admit of any doubt; and in no case in which all the circumstances of its outbreak have been carefully investigated, has any valid evidence been discovered to countenance the supposition of the importation of the fever from abroad.

It is possible, we admit, that the arrival of a foul vessel, or one with damaged articles of certain kinds on board, during a season of extreme heat, may give rise to yellow fever. In such a case, however, the disease will be confined entirely to the crew of the vessel and those who have direct intercourse with her, or to such as reside so near to the wharf at which she lies as to be within the influence of the poisonous emanations from her hold, while new cases of the fever will cease to occur on shore so soon as she is removed. We cannot conceive of the possibility of an epidemic of yellow fever being induced in any locality, and there continue to prevail for weeks and months, simply by the arrival of an infected vessel.

That the yellow fever which prevailed during the summer and autumn of 1853, within a very limited district of this city, was produced by local causes alone, is a fact established by the most conclusive evidence; and that the same is true in relation to the more wide-spread epidemic which, at the same time, ravaged New Orleans and its vicinity, Dr. Fenner has, in our estimation, very clearly proved in the work before us. He has, with great industry and candour, and apparently with the utmost care, investigated all the circumstances calculated to throw light upon the origin of the disease in New Orleans, and has, we think, very clearly shown that they are adverse to the supposition of its importation from abroad, while they indicate with great clearness its dependence upon causes existing within the city itself.

It is not our intention to present an outline of the history of the epidemic yellow fever of New Orleans of the last year, as given by Dr. Fenner, nor any abstract of the evidence he adduces bearing upon the question of its domestic origin. We prefer that the reader who feels an interest in the subject should study the facts collected by Dr. Fenner in detail, as the best means of enabling him to judge of their relevancy, and of the correctness of the conclusions which the author deduces from them.

The character of the facts, and of the deductions the author has made from them, will be seen by the following general summary, which appears on page 72 of the history under consideration.

"1. We have shown that, when the pestilence broke out, the condition of the city in respect to *cleanliness* was so bad as to be an object of *public notoriety*. Indeed, it was in such a state that, if it had given rise to Egyptian plague instead of yellow fever, it should not have surprised any one. The only surprising thing is, that, with so much filth of all kinds as is always to be found in this city, we do not have an epidemic every year. The fact that *we do not*, has even led to the public expression of the strange opinion, that the public filth of the city, instead of *originating* yellow fever, absolutely *protects* us from it in no small degree.

"2. We have shown that yellow fever appeared much earlier this year than usual, and that the *first cases* occurred in persons who had been in the city but a few days, in one instance six days, and in another thirteen. These were unacclimated persons, who had just arrived from Europe, and came over in vessels direct from Liverpool and Bremen, without stopping at any place where yellow fever was prevailing.

"3. We have shown that no vessel that arrived from any *infected* port, whether in South America or the West Indies, brought any cases of yellow fever; had any on board them whilst they were here, or gave rise to any cases previous to those which must have originated in this city.

"4. That within a very few days after the occurrence of the first cases, others were seen in different and remote places, having no sort of connection or intercommunication that we were able to trace after a careful inquiry. That shortly afterwards other cases occurred in still more remote and separate localities, under like circumstances.

"5. We have shown that the disease was the same that prevails here to a greater or less extent every year, differing only in the extraordinary degree of its malignancy.

"6. We have shown that all the varieties of endemic fever which prevail throughout the southern country were seen here in the midst of the epidemic yellow fever, and they were seen to interchange types just as is observed among the fevers of the country."

To this summary Dr. Fenner appends the following comments:—

"From these facts I am led to the inevitable conclusion, that the disease we have been considering originated from local causes existing in this city, aided by an extraordinary constitution or condition of the atmosphere at the time.

"I am not prepared to maintain that the putrefying filth which is to be found to a great extent in this city at all times, is the *sole cause* of yellow fever; for, if it were, we should certainly have an epidemic here every year—but I do believe it is almost a *sine qua non*, and that if it were effectually removed, and the whole city well paved, it would effect more towards the *prevention* of yellow fever here than anything that can be devised. This opinion is surely well supported by the established facts that *yellow fever always breaks out and rages worst in the unpaved and least improved parts of the city*, whilst *the well paved and best built portions generally suffer least from it.*

"It is often stated, and believed by many, that yellow fever always breaks out first among the shipping. *This is not true*, as I have repeatedly demonstrated in my previous writings on the subject. It is firmly believed by many persons that New Orleans is indebted to its naval intercourse with the West Indies, Vera Cruz, or Rio Janeiro, for all the yellow fever that is ever seen here. In opposition to this opinion stands the stubborn fact, that this intercourse has been continued, with but slight interruption, for half a century past, whilst we have only had epidemics of yellow fever on an average of about once in every three years. And furthermore, I have, upon several occasions, carefully noted cases of this disease which were imported from Havana and Vera Cruz, and although they were placed in apartments where they were surrounded by persons who had never had yellow fever, I never knew the disease to be communicated in a single instance.

"The great epidemic of this year certainly broke out first in New Orleans, but the peculiar constitution of the atmosphere, upon which I must think it in a great measure depended, extended to a greater circumference than was ever known before in this region. Still, it had its bounds, and nearly all places within those bounds which had local causes of disease capable of generating yellow fever, with the aid of this extraordinary constitution of the atmosphere, did have it. The range of this yellow fever atmosphere, although so wide, was as well defined as the narrow locality in the city of Philadelphia, in which the disease originated and was confined last summer."

We shall notice in this connection the general statement of Dr. Fenner in relation to the meteorological condition of New Orleans preceding and during the prevalence of yellow fever, and his remarks on the close of the epidemic, both of which occur in a previous portion of the history.

After giving an abstract of the state of the weather from the register kept by Dr. E. H. Barton, of New Orleans, Dr. Fenner remarks:—

"From this abstract we learn that the winter (of 1852–53) was quite cool and dry, but without any very cold weather. I did not see any ice in the open gutters of the city.

"The spring was quite variable; alternately hot and cold, and rather dry. Vegetation *remarkably backward*.

"From the 12th to the 20th of May the days were *very hot*, whilst the nights were so cold as to render one or two blankets necessary to comfort. The thermometer rose to 88°, which is within one degree of the highest point in July. On the 21st there was a sudden change, the weather becoming quite cold. This continued for two or three days; and *it was about this time*, as well as I have been able to ascertain, *that the few cases of yellow fever appeared* in the city.

"The summer was hot, though *not unusually so*. It will be seen that the *average* of the thermometer was above 79°, which some contend is necessary for the prevalence of the yellow fever as an epidemic. The summer was likewise *quite wet*, though not as much so as others in which there was not so much yellow fever."

From this statement it would seem that the combined influence of heat, moisture, and filth, are not alone sufficient to produce an epidemic of yellow fever.

Treating of the decline of the epidemic of 1853, Dr. Fenner remarks:—

"October 13. The Board of Health announce publicly, that *there is now no epidemic in the city, and that absentees and strangers may come in with safety.* This announcement was welcomed with joy, and from this time people rushed in from all quarters. A few were attacked after their arrival here, but not severely, the malignancy of the fever having been greatly mitigated.

"I stated, in my memorandum of the 17th September, that I considered the epidemic was really over then, although there was no public announcement to that effect. My opinion was based, not *alone* upon the great diminution of cases, but also upon *the change of type* that was visible in the new cases then occurring. Fevers of the *pernicious, intermittent*, and *remittent* types became more common than yellow fever. In fact, the yellow fever type was gradually giving place to the remittent and intermittent. This is what always occurs, as I have completely demonstrated in my previous writings on the subject.

"The number of deaths from yellow fever, for the week ending September 17, was 221. Now, if we refer to the period when the deaths first got up to a similar amount, we shall find that it was the week terminating the 16th of July, when the deaths were 204. Thus we have *two months* as the period during which it may be said *yellow fever prevailed as an epidemic*. This again corresponds with the observations of the past. We have more or less yellow fever in this city every year, but, after a constant residence here of twelve years, I must say that I have never known it prevail *to an epidemic extent* for a longer period than *sixty or seventy days*."

"A popular error, of extensive prevalence and long standing, respecting yellow fever, is, *that an epidemic can alone be extinguished by frost.* I demonstrated this error in my histories of the epidemics of 1847 and 1848, and the events of the present year fully confirm the correctness of my observations. In 1847, the epidemic ceased long before the appearance of frost, yet sporadic cases were seen to a considerable extent to the end of the year. On the present occasion, the disappearance of the epidemic was published by the Board of Health on the 13th of October, whilst the *first frost* was announced on the 25th of October, and that was only observed on the outskirts of the city. On the 31st there was a much heavier frost, but by this time a large number of persons had returned to the city. It is true that some of them were attacked with the fever, as before stated, but the cases were generally mild, and easily managed. We shall doubtless continue to see scattering cases till the end of the year. The general remote cause of all the endemic fevers seen here, which, in its most virulent or powerful state, produces an epidemic of yellow fever, gradually undergoes modification in the progress of time; it becomes less virulent or powerful, and in proportion to the degree of this change will it produce the milder types of fever." "The prevailing *fever cause*, which in August produced *yellow fever* in nine-tenths of the cases which occurred, had become so much weakened in October as to produce mostly the *remittent and intermittent types;* yet the relationship between them was still so close, that if the latter were not promptly cured, they ultimately presented the worst features of the former."

It is now very generally admitted, particularly by the leading American physicians who have studied the yellow fever under circumstances the best adapted to lead to accurate conclusions on the subject, that the disease is non-contagious—that when a patient affected with it is removed beyond the locality where it prevails endemically to a healthy situation, he will not communicate the fever to those who surround him. That this is generally almost universally true in reference to yellow fever is strongly insisted upon by Dr. Fenner—and yet we find him making the following unaccountable admission:—

"I am not prepared to maintain that yellow fever is *never* communicated, *per se*, from person to person. On the contrary, I can readily conceive that, like cholera, typhus, and dysentery, it may sometimes display infectious or contagious properties to a limited extent. I have *never seen* an instance of the kind; but cases have been reported to me, upon such *reliable testimony*, that I could see no other way of accounting for them than direct communication."

"Again," says Dr. F., "yellow fever may be contracted in an infected district, and conveyed in the person of the sufferer to a place quite remote."

Now we find, in the history which Dr. F. has given us of the epidemic which prevailed in New Orleans and its neighbourhood, during the summer and autumn of 1853, no facts to warrant the foregoing admissions, which are, also, in direct opposition to the experience of those who have carefully observed the disease in other epidemics occurring in other localities. It is true, Dr. F. endeavours to weaken the force of his admissions by showing that the contagiousness of yellow fever is a rare and accidental concomitant of the disease. "Admitting it as true," he remarks, "that yellow fever is capable of regenerating itself in the bodies of the sick, and of thus extending from person to person in close proximity, let us pause a moment and examine *the value* of this fact. At all the places where yellow fever prevails in the United States, it appears at *distinct intervals* of different lengths. It breaks out and rages for a season, disappears entirely, and, after a lapse of time, returns again. When it reappears, how often can it be traced, with *reasonable probability*, to infected vessels, goods, or persons? If this can be done in a *considerable majority* of instances, it will be sufficient to make *a rule*, and the instances in which such a connection cannot be fairly traced, must be set down as *exceptions* to the rule. Now I maintain, after careful observation of the outbreaks of yellow fever at New Orleans for the last twelve years, that no such connection could be reasonably traced in any instance. Nor has it been traced in a satisfactory manner in Mobile, Natchez, Vicksburg, or any other place in the United States where this disease has often prevailed. Hence, the *great majority* of the physicians, and others who have had the best opportunity of observing, have come to the conclusion that epidemics of yellow fever *do not usually, if ever*, originate from this source. This is also the uniform conclusion, with but *few exceptions*, of all those who have seen most of the disease in all parts of the world where it prevails, as any one can see who will examine the volumes of testimony that have been published on the subject."

Again, in speaking of the conveyance of the fever by those affected with it to places remote from that in which it was contracted, Dr. Fenner says:—

"In such instances"—that is, the removal of the sick to unaffected districts—"which are very frequent, and doubtless familiar to thousands of persons in this region, *how often* is the disease communicated to those who come in contact with it? I will venture to say, *not as often as once in fifty cases.* Many would say, not once in a hundred. Two highly intelligent gentlemen of Memphis, have recently told me that probably not less than seventy-five cases of the malignant yellow fever of this year were landed from the steamboats at that place, and there died, or recovered, without communicating the disease in a single instance. Dr. Jewell, of Philadelphia, informs us that eleven cases of yellow fever were admitted from the infected district into the Pennsylvania Hospital, and three into the Blockley Almshouse; they were mixed promiscuously with other patients, yet the disease was not communicated in a single instance. Dr. Ball, of this city, who on behalf of the Howard Association, visited the village of Providence when the epidemic was at its height, tells me of several instances in which the disease was taken back to the plantations without being communicated. I will not weary the reader with instances of the kind.

"If, then, it be true that yellow fever is not communicated from one person to another by immediate presence or contact, independent of other influences, oftener than *once in fifty or a hundred exposures*, it would certainly make but a *rare exception* to a general rule; and this I believe to be about the true and real value of the now popular and warmly advocated doctrine of the *contagiousness of yellow fever*. It is equally applicable to *fomites* of all kinds."

All that we know of unquestionably contagious diseases, proves, that their capacity to propagate themselves by a specific poison, generated in the systems of those affected with them, is their invariable characteristic. We know of no facts that countenance, in the slightest degree, the idea that a disease, admitted to be, ordinarily, of malarious origin, and which, we are assured, is ever known to spread in a healthy district into which those labouring under it are introduced, may occasionally assume a contagious character. The fact, which cannot well be contested in the face of the mass of incontrovertible testimony which has been accumulated in its support, that the yellow fever originates in local morbific causes, altogether independent of any contagious principle derived from those already affected with the disease, and strictly limited to districts more or less circumscribed, is, to us, conclusive evidence that under no circumstances does it assume a contagious character. The few instances that have been reported in which the fever has appeared to be communicated by the sick to the well, or conveyed by the infected to distant and healthy localities, are all capable of an easy explanation, without the necessity of resorting to the absurd notion of a contingent and occasional contagiousness.

In regard to the characteristic features of yellow fever, so strongly insisted upon by many writers on the disease, Dr. Fenner remarks:—

"The general impression derived from reading descriptions of yellow fever is, that it is *a violent fever of a single paroxysm, lasting about seventy-two hours, and presenting strongly marked characteristic symptoms, by which it may readily be distinguished from all other types of fever.* I have not found it so; nor have I yet found the man who could always say correctly whether a case, examined *per se*, was yellow fever or not. I have already shown that differences of opinion were expressed about the first cases that appeared this year, and have only to add that the same thing occurs here every year. The truth is, yellow fever is so closely allied to various remittent and intermittent fevers, that no uniform and reliable distinction can be drawn between them in the early stages; and the only way we get at the fact that yellow fever is prevailing, is by observing the *final results*, whether in *death* or *convalescence;* and the former is by far the most conclusive of the two. Even in such an epidemic as this, thousands of cases occurred which no one would have thought of calling yellow fever, if it had not been generally known that many of the same character and appearance had terminated in fatal black vomit."

Dr. F. notices as a feature of the epidemic of 1853, *a tendency* to relapse, which presented itself in many instances:—

"After the customary subsidence of the fever at the end of the third day, there would be a period of calmness, lasting from twenty-four to forty-eight hours, and then the fever would kindle up again, and last for one day or more. This secondary fever was often very dangerous, though it frequently terminated happily. The interval of convalescence after the first attack was sometimes so long as to appear more like a *second attack* than a *relapse.* From two to four weeks have been known to elapse between two distinct attacks of the fever in the same person. Heretofore, those relapses, or second attacks, were not at all common."

In regard to *second attacks* of yellow fever—that is, in persons who have had the disease in former years, we are told by Dr. Fenner, that—

"There can be no doubt that if a person have a plain attack of yellow fever during the prevalence of a *severe* epidemic, there will be but small probability of his ever having it again, provided he remains in the same place; yet the rule is by no means *invariable.* If one has it, however, during a *mild* epidemic, or when there are only a few sporadic cases, his subsequent exemption will not be near so great. Thus, there were numerous attacks this year among persons who had other attacks since 1847, but not many among those who had it that year, or any previous strong epidemic."

Dr. F. notices a number of second attacks that occurred during the epidemic of 1853, either under his own observation or that of his medical friends; in many of these cases, however, the fact of the first attack is based solely upon the patients' own statements, upon which, in all instances, the fullest reliance cannot be placed.

In reference to the supposed immunity enjoyed by the *creoles or natives of New Orleans* from an attack of the fever, Dr. Fenner states, that—

"One of the most extraordinary features of the epidemic of 1853, is presented in the fact that the natives of the city, *both white and coloured,* have suffered severely, and many of them died of it. This is generally admitted and beyond dispute. Children who were born since 1847 have suffered most; but many born previous to that time likewise suffered, and some of them died of black vomit. I saw two quadroon girls, sixteen and seventeen years of age, who were natives, and had never lived anywhere else—they had the fever severely, but recovered. A child, aged five years, in the same family, also had it. I attended a quadroon boy, about eight years of age, who had a severe attack, attended with hemorrhage from the gums. I have already mentioned the case of a white infant, only *five weeks old,* that died of black vomit. It may be stated generally that all children and young persons in the city, who had never had yellow fever previously, were attacked this year. Of course, there were some exceptions to this, as there are to every general rule.

"I am informed that the creoles on the coast, above and below the city, suffered much worse from yellow fever than those in the city, many adults having died of the disease."

It appears that during the epidemic of 1853, *unacclimated* negroes, or those who had never had yellow fever before, were equally as liable to attacks of the disease as the white population, but the disease was not so severe and fatal in them as in the whites generally.

"This is equally true," Dr. F. remarks, "of the mixed races generally. It is a well-established fact that there is something in the negro constitution which affords him protection against the worst effects of yellow fever; but what it is, I am unable to say. During an epidemic, he will take the fever almost, if not fully as readily as the white, but it will be altogether milder and less dangerous in its tendency. In short, it will correspond more exactly with the bilious remittent fever that prevails in the country, and requires precisely the same treatment. And yet this type of fever in the city negro must be produced by the very same cause that gives rise to malignant yellow fever in the white race. Occasionally, we see the hemorrhagic diathesis of yellow fever displayed in the negro—but it is by no means common. The least mixture of the *white race* with the *black* seems to increase the liability of the latter to the dangers of yellow fever; and the danger is in proportion to the amount of white blood in the mixture. Very few negroes ever die of yellow fever in this city; but I learn that a considerable number have been lost on the plantations this year. The cause of this may readily be imagined, when it is recollected that yellow fever never prevailed on the plantations before, and of course most of the physicians were not familiar with its treatment.

"I consider the danger to negroes from yellow fever to be no greater than that from *bilious fever* in the country, and *not half so great as from congestive intermittent.*"

Dr. Fenner estimates the chance of *recovery, when well-marked black vomit* has occurred in a case of yellow fever, to be but little better than one in a hundred. He nevertheless presents quite a respectable list of such recoveries, which during the epidemic of 1853, occurred under his own observation and that of other physicians of New Orleans.

We close our notice of Dr. Fenner's history by the following extract, exhibiting his experience, during the late epidemic, of the "abortive treatment" of yellow fever by large doses of quinia, so highly extolled in some of his previous writings:—

"In 1847, and ever since, till the present season," he remarks, "I have been able *promptly to cut short* nearly all the cases of yellow fever I was called to treat in this city; and there was but one year (1851), in which there was not a good deal of it to be treated. Several other highly intelligent physicians of this city have done the same. Our chief remedies for this purpose were large doses of quinia and opium, given at the onset of the fever. In reporting my satisfactory success with this treatment, I admitted that the disease treated, although certainly yellow fever, was of rather a milder type, and that it remained to see whether this abortive treatment would be equally successful in a more malignant epidemic. The opportunity has been presented this year, and I must make a candid statement of the results of my experience.

"When this epidemic broke out, I was one of the visiting physicians to the Charity Hospital, and soon had ample opportunities to test the abortive treatment by large doses of quinia and opium. It did very well in many cases where there was a *fair opportunity to apply it*, but I soon discovered that it did not display that controlling influence over this fever which it had done over all the yellow fever I had met with for six years previously. I then fell upon a more moderate use of the sulphate of quinia, and finally gave preference to the *ferro-cyanuret*, in combination with blue mass, and without opium or morphia. I was pleased with the results, and pursued this course to the end of the epidemic.

"I do not recollect to have treated any case of yellow without giving quinia in some form, and am willing to compare results with any physician in the city. I still believe it to be one of the most, if not *the* most valuable of all our remedies in yellow fever—in short, that it is just as valuable in this type of fever as it is in bilious remittent. Many physicians tell me that they found the sulphate of quinia to fulfil their expectations this year as well as usual, whilst others report quite differently." D. F. C.